The Unintended Consequences of Employer Credit Check Bans on Labor and Credit Markets

Kristle Cortés

University of New South Wales

Andrew Glover

University of Texas at Austin

Murat Tasci

Federal Reserve Bank of Cleveland

_____

The Unintended Consequences of Employer Credit Check Bans on Labor and Credit Markets

Kristle Cortés, Andrew Glover, and Murat Tasci

The Unintended Consequences of Employer Credit Check Bans on Labor and Credit Markets

Kristle Cortés, Andrew Glover, and Murat Tasci

Since the Great Recession, 11 states have restricted employers' access to the credit reports of job applicants. We document that county-level vacancies decline between 9.5 percent and 12.4 percent after states enact these laws. Vacancies decline significantly in affected occupations but remain constant in those that are exempt, and the decline is larger in counties with many subprime residents. Furthermore, subprime borrowers fall behind on more debt payments and reduce credit inquiries postban. The evidence suggests that, counter to their intent, employer credit check bans disrupt labor and credit markets, especially for subprime workers.

"We want people who have bad credit to get good jobs. Then they are able to pay their bills, and get the bad credit report removed from their records. Unfortunately, the overuse of credit reports takes you down when you are down."   --Michael Barrett (State Senator, D-Lexington, MA).

1) Introduction

During the last twenty years, credit-reporting agencies have found a new market for credit reports: employers deciding whether to extend a job offer to an applicant. The three largest credit reporting agencies (Experian, Equifax, and TransUnion) currently offer the service, and a 2009 survey of human resource managers at Fortune 500 companies found that 60 percent used credit reports in hiring decisions (Society for Human Resources, 2012). Additionally, a 2012 survey by the policy group DEMOS found that 25% of low-to medium-income households reported having their credit checked for a job application, and 10% claimed to have been denied a job because of bad credit (DEMOS, 2012).

In response to high unemployment and worsening credit conditions during the Great Recession, lawmakers introduced legislation to limit employer credit checks at the city, state, and national levels.[2] Eleven states have banned employer credit checks as of January 2018, the geographic distribution of which can be seen in figure 1. Lawmakers voice concern that employer credit checks may create a poverty trap. Brad Lander, who sponsored a 2015 credit-ban bill passed by New York City, provided a typical explanation for introducing the legislation: "Millions of Americans who have bad credit, would also be great employees," he said. "What they need to repair their credit is a job, and to make it harder for them to get a job is the definition of unfair" (Vasel, 2015).

In this paper, we estimate the response of key labor and credit market outcomes to the implementation of employer credit check bans. When a state bans employer credit checks, the average county experiences a substantial fall in vacancy creation relative to trend, by about 12 percent. This decline in job creation is likely caused by the bans, since vacancies are unaffected in occupations that are exempt, but fall significantly in occupations subject to restrictions on employer credit checks. Furthermore, within states that pass a ban, vacancies decline by more in counties in which more subprime borrowers reside.

---

[2] While these laws typically restrict the use of credit checks without necessarily banning them outright, we will refer to them as "bans" for expositional simplicity.

If credit check bans cause a deterioration in the labor market, then we may expect spillovers into credit markets, either because people have less income with which to pay their bills or because their incentives to repay are weakened. A novel result in our paper is to document this spillover by studying individual credit reports. We find that sub-prime borrowers' reports show more delinquencies and fewer new credit inquiries after the ban, which is consistent with these individuals being less able (or willing) to make payments or access new lines of credit. Furthermore, we find that Black borrowers are distinctly affected by employer credit check bans. While subprime borrowers of all races make fewer credit inquiries following the ban, Blacks form the one group for which delinquencies *do not* increase. Taken together, our results raise considerable doubts that credit check bans have had their intended effects of improving labor and credit markets; their effects have likely been negative.

Our estimates amount to a positive test of recent theories of the interaction between credit and labor markets. Donaldson, Piacentino and Thakor (2017) posit a theory in which debt overhang suppresses vacancies by raising workers' reservation wages. We find evidence that labor demand does, in fact, depend on the information in credit reports, of which debt is one part. More directly related to our policy-based identification strategy, Corbae and Glover (2017) develop a screening model in which employers use credit reports in hiring because repayment rates are positively correlated with an unobservable component of worker productivity.[3] They show that banning employer credit checks may reduce vacancies, since employers cannot screen workers ex-ante.[4] Furthermore, their theory implies that employer credit check bans reduce the reputational incentive to repay debts, which increases default rates; our finding that subprime delinquency rates increase following the ban provides support for this feedback mechanism.

Our empirical approach leverages the staggered implementation of bans to estimate a difference-in-difference regression model. The labor market effects are primarily estimated using

---

[3] Corbae and Glover generate this correlation through worker heterogeneity in patience and unobservable effort in human capital accumulation. More patient workers have more (unobservable) human capital and higher repayment rates, since both require a costly investment in the present and accrue benefits in the future. In reality, employers may value workers with good credit for many reasons. As long as employers think that credit has signaling value, regardless of its actual correlation with worker productivity, one can generate such a link between both labor and credit markets.

[4] Our estimates show a decline in vacancies on average, but are silent about the effects on individual workers. Friedberg, Hynes, and Pattison (2017) find that people who report financial distress experience a reduction in unemployment duration following employer credit check bans. Their data does not have individual credit reports, so they must proxy using survey responses.

county-level observations from the Conference Board's Help Wanted Online (HWOL) data, measuring vacancies. We also use unemployment rate data from Local Area Unemployment Statistics (LAUS) program and aggregated data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (FRBNY CCP/Equifax) credit panel.[5] We estimate the average effect of an employer credit check ban using the change in log of vacancies (job postings) in a treated county (relative to trend) versus the change for an untreated county.[6]

Understandably, state and local governments are passing these laws for a reason and our identification strategy must address the concern that the same factors that affect whether a state enacts the ban could also affect the outcomes that we are measuring. We are particularly concerned with choosing an appropriate control group and verifying that treated and untreated counties look similar prior to the ban.

We address this in three ways. First, we leverage within-state heterogeneity in exposure to bans by occupation (exempt versus affected) and county-level shares of residents with sub-prime credit scores. The concerns about endogeneity apply to all job postings and applicants post-ban, so by splitting the sample in this way supports a causal interpretation of the ban. Second, we estimate the dynamic effects of bans, before and after they are enacted. We find no evidence of divergent trends in counties before the ban goes into place, again supporting a causal interpretation. Finally, we estimate our baseline specification for border counties, which allows us to control for arbitrary unobservable trends in local labor market conditions shared between adjacent counties. Each case yields similar estimates to our baseline specification.

We follow a similar approach for credit market outcomes, but here we rely on individual borrower-level observations from FRBNY CCP/Equifax. This dataset provides us with a rich set of credit variables such as credit inquiries, credit scores, usage, and delinquencies. We also estimate the effects on credit variables by racial group, for which we use the TransUnion/Epsilon credit panel. This unique data set has credit report data from TransUnion, merged with self-reported demographic data from the marketing firm Epsilon.

---

[5] Federal Reserve Bank of New York Consumer Credit Panel/Equifax data is a subset of credit data maintained by Equifax, one of the large credit reporting agencies. The data are from a 5 percent sample of all individual credit records that Equifax maintains.

[6] We refer to a county as treated during a given quarter if it is located in a state with employer credit check bans in effect at that time. We estimate the model both statewide and using only contiguous counties.

Several studies study the effects of employer credit checks on labor market outcomes. To our knowledge, ours is the first study to analyze the effect of employer credit check bans on local labor demand (i.e. job postings). We are also the first to consider the effect of these bans on credit markets and thereby highlight a hitherto overlooked effect of the policy. We are among the first to study the effect of these laws on labor market outcomes *in general*, though two recent papers by Ballance, Clifford and Shoag (2016) and Bartik and Nelson (2016) are closely related. While they consider different outcome measures and implement different empirical strategies, these studies also find that employer credit check bans have negative labor-market effects on their targeted populations.[7]

Ballance, Clifford and Shoag (2016) estimate the effect of bans on log-employment at the census-tract level, using annual data. Substantively, our labor market estimates differ in two ways. First, we use data on vacancies, unemployment, job-finding rates, and separation rates as our labor-market outcomes, rather than total employment. The ban's effect on vacancies and labor-market flow rates are more salient than employment since they are directly related to individual's probability of being employed. For example, total employment may rise if the law motivates labor-force entry or immigration to the state post ban, which is nonetheless a negative outcome for the average worker if vacancies do not rise in proportion. Second, our use of quarterly data allows for an extremely accurate coding of the effective date of bans.

Bartik and Nelson (2016) use the panel dimension of the Current Population Survey (CPS) as well as data aggregated from state unemployment insurance records to estimate the effect of employer credit check bans. They are primarily focused on the average flow rates for different racial groups in the labor market and only find conclusive results for blacks. They report that bans reduced job-finding rates and increased the separation rates for blacks significantly. These results are consistent with the main findings in our paper. By estimating the effect of credit check bans on the vacancies and the spillover into credit-markets, we complement the conclusion of Bartik and Nelson (2016).

Our paper is also related to several recent papers that study the interactions between the labor market and the credit markets, especially via the use of credit market information, e.g.,

---

[7] These groups include individuals with mid to low credit scores, young workers, and blacks.

Bos, Breza and Liberman (2015); Herkenhoff (2015); Herkenhoff, Phillips and Cohen-Cole (2016); and Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016). The most relevant comparison is Bos, Breza and Liberman (2015), who study a regulatory change in Sweden that removed negative information (bankruptcy, defaults) from some borrowers' credit reports. They find that this change led to higher employment rates for the affected groups. Though the affected group is a limited segment of the population (previously defaulted pawnshop borrowers) in a different country (Sweden), their results confirm that credit market information can affect labor market outcomes.

In the context of American credit and labor markets, Herkenhoff, Phillips and Cohen-Cole (2016) and Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016) use the removal of the individual bankruptcy flag from consumer credit report as an instrument to estimate the effect of credit worthiness on labor market outcomes. The former study finds that bankruptcy flag removal affects labor supply: As credit terms improve, displaced workers take longer to find jobs and receive higher wages upon reemployment. Our estimated increase in unemployment rates is consistent with their results, though we estimate insignificant effects on earnings.

Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016) rely on the differential effects of the flag removal on labor market outcome variables for Chapter 13 filers relative to Chapter 7 filers. A Chapter 7 filer's default flag appears on her report for ten years after bankruptcy, while a Chapter 13 filer's flag is removed after only seven years. Based on outcomes for Chapter 13 filers within the three-year window after which their default flag is removed, Dobbie et al. estimate zero effects on employment and earnings and conclude that labor demand is insensitive to credit worthiness. Contrary to their estimates, we find significant effects in the labor market in response to credit check bans, especially for our more direct measure of labor demand, vacancies. We reconcile our results with theirs by noting that a seven- to ten–year-old bankruptcy flag may provide employers with little significant information about a potential hire since life-cycle components of labor productivity and other observable labor market experiences during the first seven years after bankruptcy likely swamp any signal provided by the bankruptcy flag.[8]

---

[8] Moreover, public sector employers in the United States are not allowed to use bankruptcy filings in hiring decisions. If, at the margin, employers with government contractors respond to this constraint by hiring or retaining

2) Data and Empirical Approach

Table 1 details the timeline of law changes across states and Figure 1 maps the states that currently have laws in effect as of April 2017. Throughout our empirical analysis, we focus on the period 2005:Q1 through 2016:Q4. We use the date at which the law became enforceable to code our treatment flag, with the convention that dates falling within a quarter are coded as the beginning of that quarter (8 of 11 states began enforcing their bans at the start of a quarter). The resulting summary statistics for this flag are seen in the last columns of table 2. More than 10 percent of counties are affected by the credit check bans at the end of our sample period (out of 3,137) covering about 26.5 percent of the U.S. labor force.

a) Labor Market Data

Our principle labor market outcome is the county-level vacancy (job opening) data reported by the Conference Board (2017) as part of its Help Wanted OnLine (HWOL) data series. HWOL provides a monthly snapshot of labor demand at detailed geographical (state, metropolitan statistical area, and county) and occupational (6-digit SOC and 8-digit O*Net) levels since May 2005.[9] For the period in question, HWOL represents the bulk of the advertised job openings, as print advertising ceased to exist.[10] HWOL covers roughly 16,000 online job boards, including corporate job boards, and aims to measure unique vacancies by using a sophisticated deduplication algorithm that identifies unique advertised vacancies on the basis of several ad characteristics such as company name, job title/description, city, or state. HWOL is not the only source of data on job openings, though. The Bureau of Labor Statistics (BLS) publishes nationally representative data, the Job Openings and Labor Turnover Survey (JOLTS), which measures also vacancies. However, HWOL's detailed geographic- and occupation-level coverage makes it uniquely suitable for the empirical question we have in mind.[11]

---

workers with a bankruptcy flag to ensure compliance, then the estimated effect of credit on labor demand will be biased towards zero.

[9] For a detailed description of the measurement concepts and data collection methodology, please see (The) Conference Board. (2017). *The Conference Board Help Wanted OnLine® (HWOL) at* https://www.conference-board.org/data/helpwantedonline.cfm.

[10] In fact, HWOL started as a replacement for the Conference Board's Help-Wanted Advertising Index of print advertising.

[11] JOLTS' publicly available data files do not have more detailed coverage than census regions and lacks any information on occupational characteristics. For most of the sample period, the general patterns reported in JOLTS and HWOL are reasonably close to each other. See, for instance, the relevant discussion in Sahin, Song, Topa and

Figure 2.a displays the average level of vacancies over time for two groups of states: treated and untreated.[12] On average, vacancies are higher in treated states, the list of which includes some populous states such as California and Illinois. Vacancies are procyclical for each group. Table 2 displays summary statistics for outcome variables at the county-level, the level of observation we use in our regression analysis. The average county seems to trend up during the sample period, briefly interrupted with a minor decline from 2008 to 2009. Taken together, these statistics suggest a large variation in the vacancy data across counties and states.[13]

We prefer vacancies as our labor market measure, since it is closest to labor demand and allows us to exploit variation by exempt and affected occupations, but we are also interested in broader labor market variables. Specifically, we measure the change in unemployment, job-finding, and separation rates following a ban. We use county-level unemployment rates, as reported by the Bureau of Labor Statistics' LAUS program.[14] The job finding and separation rates we use are computed from CPS microdata at the state level. Since respondents in the survey are interviewed repeatedly for certain months one can create a panel from the observed transitions of workers.[15]

Table 2 shows the summary statistics for unemployment rate on an annual basis. There is a clear rise in the average level nationwide throughout the Great Recession, as along with an

---

Violante (2014). Researchers identified a recent diversion between vacancy measures across these two sources, one which is attributed to a change in pricing on several online job boards (Cajner and Ratner, 2016). To the extent that fixed effects in our empirical specifications absorb these pricing changes, our results will be immune to significant bias by relying on HWOL.

[12] Note that the set "Ever Treated" includes all 11 states that had, at some point during the sample period, a credit check ban in effect. Similarly, the "Never Treated" group consists of the remainder of the states. Hence, each line has the same set of states consistently, even though states enacted their laws on different dates. The averages plotted in the figures use each state's labor force to weight the relevant labor market variable.

[13] The exact specification is of log of (vacancies +1) and the summary statistics highlight how this varies little from log of vacancies without keeping the zeroes in the observations. We choose to keep the zeroes because some occupations periodically post listings in a given county, but not in every period. The estimated effects from regressions with the log of vacancies as an outcome are similar to those reported here and are available upon request.

[14] We also have county-level observations for employment and labor force through LAUS. All these estimates for counties are produced through a statistical approach that also uses data from several sources, including the CPS, the CES program, state UI systems, and the Census Bureau's American Community Survey (ACS), to create estimates that are adjusted to the statewide measures of employment and unemployment.

[15] Each responded is interviewed for four months initially and then leaves the survey for eight months. They are interviewed again for four more months. For any given month, about 70 percent of the survey respondents are observed consecutively allowing us to create a panel to measure average job finding and separation hazards. Unfortunately, due to the size of the CPS and number of transitions, we cannot obtain estimates for more granular level than state. We follow Nekarda (2009) to minimize the bias induced by mobility.

increase in the standard deviation across counties during this same period. The best source of household-level labor market data is the CPS. Unfortunately, sample size becomes relatively small at the county level for most counties in the U.S. This feature of the CPS limits us to LAUS for the county-level data on unemployment rates. Nevertheless, as in Bartik and Nelson (2016), we also use the CPS to get state-level unemployment rates in addition to job-finding and separation rates.

For our purposes, the relevant source of variation in unemployment is between counties in states that have enacted a ban at any time in our sample period and those that have not. This can be seen in figure 2.b, which plots the average unemployment rate by treated and untreated states over time Until the Great Recession, the unemployment rates in treated and untreated states were quite similar, but a gap began to appear in 2007:Q3 and grew throughout the recession. As the vertical lines indicate, most states enacted their credit check bans after the Great Recession. The difference between treated and untreated states was nearly 2 percentage points in 2010 and has only recently begun to shrink. We certainly cannot conclude from this plot alone that the laws have caused this decline, but it offers additional illustration of our findings.

The heterogeneity in unemployment rates between states with the ban and those without can be further understood by comparing job flows. Specifically, figure 2.c compares the job-finding rate between these two groups, and figure 2.d compares the separation rate. Starting around the same time as the divergence in unemployment rates, the job-finding rate is slightly lower in the states that have banned employer credit checks, but this rate alone is not enough to account for the difference in unemployment rates. The larger difference is between separation rates, which may mean that short-term employment spells have taken the place of credit checks as a screening device.

b) Credit Market Data

The FRBNY CCP/Equifax panel provides detailed quarterly data from Equifax on a panel of U.S. consumers and includes Equifax Risk Scores (credit scores) and other data on consumer credit reports. We use the data on consumer credit scores and estimate the effect of the ban as a function of the credit-score conditions within a county. The distribution of subprime borrowers

across counties and over time is found in table 3. For this paper, we follow the literature and assume that the critical level for being subprime is an Equifax risk score of 620. Over the sample period we analyze, the average fraction of subprime borrowers within a county was 27 percent, declining from 29 percent to 25 percent over time. The breakdown of subprime borrowers is similar for TransUnion even though Equifax and TransUnion have different credit score models (Equifax Risk Score and TransUnion VantageScore.) Additionally, at the time of our sample the TransUnion VantageScore ranged from 501-990. It now uses a range from 300-850, making it more in line with over credit scores.

There is substantial variation across counties in our sample: A county in the 95th percentile has more than 45 percent of borrowers with subprime credit scores over the sample period compared to 20 percent for a county in the 25th percentile. This variation helps us identify the differential effects of the credit check bans for subprime households, which are commonly targeted by these policies.

The rich panel structure of the credit data allows us to test the effects at the individual level. Although the credit panel contains detailed geographic and credit information, it does not contain demographics beyond age. As Ballance, Clifford and Shoag (2016) and Bartik and Nelson (2016) find, the bans had more pronounced effects on specific demographic groups. To compensate for this lack of demographic information in the FRBNY CCP/Equifax, we use the TransUnion/Epsilon Credit Panel to measure variation across demographic groups.

Epsilon is a marketing firm that collects individuals' personal data, such as age, race, income, level of education etc., which they market to firms trying to hone their advertising strategies. One such method of data collection is through a self-reported survey sent directly to the household. The final Epsilon data records when a household self-reports and the race data is based on questions regarding race and country of origin, which can be very detailed. Our use of this data is possible because TransUnion matched their credit panel data to Epsilon by name and address of the borrower and provided us with the anonymized results. Although the TransUnion data captures a representative sample of the U.S. population by selecting certain Social Security codes, it is apparent in the data from Epsilon that the same proportions of borrowers cannot be matched perfectly. The self-reported data from the 2010 U.S. Census reports that 72.4% of the

population identify as White alone, whereas 12.6% report to be Black and 16.3% are Hispanic. Of our final matched TransUnion/Epsilon sample, 10% are Hispanic but only 5% are Black.[16]

Table 4 shows that the two panels are similar for the two credit market outcomes of interest, inquiries and delinquencies. Credit data in FRBNY CCP/Equifax are measured quarterly, and the mean number of credit inquiries during the previous three months is 0.6, whereas TransUnion is measured annually and has a mean of 0.8 from credit inquiries during the last six months. Breaking the data down by race shows that blacks request credit slightly more than Hispanics and whites.

Panel A of table 4 shows that, on average, 12 percent of loans are delinquent for the FRBNY CCP/Equifax sample, whereas 10 percent are delinquent in TransUnion. Black borrowers are slightly more likely to have delinquent accounts: roughly 18 percent compared to whites at 8 and Hispanics at 13 percent.

Panel B of table 4 presents the same descriptive statistics for the group of subprime borrowers in both datasets. As expected, this group consists of borrowers who, on average, have more credit inquires and larger delinquent loan balances compared to the overall sample of borrowers. However, among subprime borrowers, there is little demographic heterogeneity in total inquiries or delinquent balances. The differences in average credit market outcomes across races in panel A are mostly explained by the fraction of subprime borrowers in each group.

Panel C of table 4 details the summary statistics for borrower credit scores. The average credit score from the Equifax Risk Score is 693 and the average TransUnion VantageScore is 732 for 2005-14. The average VantageScore for Blacks in our sample is 664, so the higher number of credit inquiries and the higher percentage of delinquent loans are consistent with a lower average score. Hispanics have better VantageScores in the sample, with an average of 701. Whites have an average VantageScore of 756 and represent the largest subsample in our data.

c) Policy Endogeneity

---

[16] This results in a smaller sample size for our tests that use the merged TransUnion/Epsilon data and may also make this sample less representative of the U.S. population. However, we think these results are interesting since previous studies of labor market responses to employer credit checks have emphasized racial heterogeneity.

When policy makers consider credit check bans, they are often worried about a poverty trap in which workers lose their jobs, causing them to fall behind on debt payments, which then makes it harder for them to find jobs because their credit rating worsens. Corbae and Glover (2017) show that such a trap may arise in a general equilibrium model. If the passage of these bans is correlated with our variables of interest, then our estimates cannot be interpreted as causal. We attempt to control for this in our panel regressions with fixed effects at extremely local levels, including a fully flexible local time trend using the adjacent-border county specification. We also test for pre-trends in an extension of the baseline specification with distributed lags in the treatment variable. In addition, we exploit the unique nature of the exemptions embedded in credit check bans for additional policy variations. These additional checks alleviate concerns about policy endogeneity for the primary variable of interest, vacancies. We leave the discussion of these formal checks for the next section and here, instead, focus on various salient labor and credit market variables in treated and untreated states over time to make the same argument.

The unemployment rate shows no difference between treatment groups until the end of the Great Recession (figure 2.b). The divergence following the recession implies a significant difference in response to the credit check bans at the county level in our empirical analysis. More interestingly, poverty rates differ substantially between the treatment groups, but inconsistently with the endogeneity concern that states pass employer credit check bans because of poor economic conditions (figure 3.a) since treated states had uniformly lower poverty rates than untreated states. Although poverty rates increased substantially after the Great Recession, treated states do not stand out as high poverty areas.

The same picture emerges if we look at median household income or the fraction of subprime borrowers across states (figures 3.b and 3.c). Median household income in all states stagnated from 2007 through 2013. Nevertheless, treated states had almost $10,000 higher median household incomes throughout the sample period. Finally, figure 3.d shows that treated states had lower delinquency rates on average than untreated states. In summary, contrary to the standard policy endogeneity concern, treated states had more favorable economic conditions than non-ban states.

3) Results

We use a county-level panel with labor market data and an individual-level credit panel to test the effects of the employer credit check bans. We primarily estimate the effect on job creation (measured by the number of help wanted ads posted online) using county-level data, and we use the credit panel to estimate the effects on credit market outcomes for individual borrowers.

a) Effects of the Credit Check Bans on Job Creation

We begin by estimating the following regression at the county level:

$$log(vacancies_{i,t} +1) = \alpha_i + \gamma_t + \beta\,Ban_{i,t} + \varepsilon_{i,t}, \qquad (1a)$$

where α_i is a county fixed effect and γ_t is a time fixed effect (measured quarterly). The coefficient of interest in this regression, β, is identified from the average growth in vacancies (HWOL data) for a county in a treated state before and after the ban was passed, relative to the national average in the same periods. The estimated coefficient for vacancies is found in column (1) of table 5: it is statistically significant and economically large, implying a 12.4% decline in vacancies after the ban goes into effect.

This large negative effect on job openings is counter to the goals of policymakers. However, we cannot observe the counterfactual – vacancies may have fallen even more in these counties if a ban had not been enacted. We attempt to address this concern by exploiting the structure of employer credit check bans: Every state that implemented these bans included some exemptions for specific occupations. These exemptions typically focus on occupations in which workers have access to financial or personal information (handling cash or access to payroll and social security information, for instance) on the job. We can therefore estimate the change in vacancies for occupations that are exempted from bans versus those that are affected.

We first divide vacancies into two groups for each county-quarter observation: exempt and affected. The exempt group includes all vacancies that the Conference Board identifies as belonging to three specific 2-digit SOC codes, Business and Financial (SOC-13), Legal (SOC-23), and Protective Service (SOC-33) occupations. The affected group consists of all other

vacancies.[17] Columns (2) and (3) in table 5 present the regression results for these cases when the dependent variable corresponds to total vacancies in exempt and affected occupations, respectively. Vacancies in exempt occupations fall nearly an order of magnitude less than in affected occupations (-1.7% versus -12.6%). Furthermore, the decline is statistically indistinguishable from zero for exempt occupations, but statistically significant for affected occupations. The difference in effect between exempt and affected occupations suggests that bans *cause* a decline in vacancies in occupations where it is enforced.

In our baseline specification described in equation (1a), our control group consists of counties across the nation that have not been treated, no matter how spatially distant they are from those with a ban. This approach might induce bias in our estimates if there are unobserved differences at the county level that are time varying. This would be the case, for instance, if vacancies exhibited time-varying differences in their trends between the treated counties and the untreated ones. This issue has long been recognized in the literature, leading researchers to focus on the adjacent counties along a state's border, effectively making the untreated neighboring county the relevant control and therefore controlling for flexible unobservable trends in local labor markets.[18]

We therefore estimate the effect of employer credit check bans using only adjacent counties across state lines. Counties are grouped along with all neighboring counties outside of their respective states to form a cross-sectional unit of observation for county *i* in pair *p*. Our treatment group consists of counties along the border of a state after that state passes a ban, and our control is all other county pairs. The effect is identified from the change in vacancies in a treated county relative to the change in its untreated paired neighbor. Following the specification of Dube et al. (2010), we link the county pair through a time-varying fixed effect and estimate the equation as follows:

$$log(vacancies_{i,p,t}+1) = \alpha_i + \gamma_{p,t} + \beta\,Ban_{i,t} + \varepsilon_{i,t,,} \qquad (1b)$$

The estimated β coefficient is found in column (1) of table 6 with standard errors clustered at both the state and border levels. It is reassuring that these estimates are significant

---

[17] Bartik and Nelson (2016) use 4-digit codes however our use of 2-digit codes would if anything make it more difficult to find a result so if anything our results are understating the true effect.
[18] See for instance Dube, et al (2010), in the context of the effects of minimum wage legislation.

and imply a similar decline to those that we observe in the nationwide estimates from equation (1a). The county-pair quarter time fixed effect means that the ban's effect is identified from the treated county's change in vacancies relative to its nontreated neighbor in each period after the ban is enacted. Our estimate implies that a county in a postban state experiences a 9.5 percent decline in vacancies relative to the decline in an adjacent county in a nonban state, a number which is only slightly smaller than the 12.4 percent decline from the nationwide estimate. As in the previous model, we estimate the ban's effect on exempt versus affected occupations separately and a similar pattern emerges: Job postings for exempt occupations are essentially constant, whereas vacancies for affected occupations decline by a statistically significant 9.7 percent (columns (2) and (3) of table 6).

These estimates show that the decline in vacancies is robust to controlling for local unobservable labor market heterogeneity. In practice, however, we must be careful when extrapolating the contiguous-county estimates to interior counties. For example, the effect may be overstated if much of the decline in vacancies within a county is due to employers' relocating to the neighboring (untreated) state. On the other hand, the effect may be understated if many workers in the treated counties were already working in the adjacent state before the ban was implemented. We therefore prefer the nationwide estimate, but take confidence from the fact that the adjacent-border-county estimates are similar.

b) Dynamic Effects of the Ban and Pretreatment Trends

Our empirical specifications expressed in equations (1a) and (1b) measure the average effect in the long-run. It is conceivable that the effects on labor demand locally might change over time following the legislation and that the long-run coefficients we reported in those specifications might change significantly. Furthermore, vacancies may have been declining in treated states prior to the treatment, even relative to our flexible trends. We therefore estimate a distributed-lags specification that captures vacancy dynamics around the implementation of bans. This approach has been found to be especially useful for studying the effects of staggered implementation of the treatment (policy change) across different jurisdictions with a difference-

in-difference identification strategy.[19] Specifically, we allow for four-quarter leads of the credit check ban and up to four-quarter lags in the following form:

$$log(vacancies_{i,t}+1) = \alpha_i + \gamma_t + \beta_{-4}\,Ban_{i,t-4} + \beta_{-3}\,Ban_{i,t-3} + \beta_{-2}\,Ban_{i,t-2} + \beta_{-1}\,Ban_{i,t-1}$$
$$+ \beta_0\,Ban_{i,t} + \beta_{+1}\,Ban_{i,t+1} + \beta_{+2}\,Ban_{i,t+2} + \beta_{+3}\,Ban_{i,t+3} + \beta_{+4}Ban_{i,t+4} + \beta_{>4}Ban_{i,t>4} + \varepsilon_{i,t}, \quad (2)$$

Typically, one hopes to estimate small and insignificant values for the coefficients indexed prior to the ban being implemented. If these coefficients were significantly negative then it would suggest that vacancies had fallen prior to the ban being implemented, raising the spectre of reverse causality. As reported in table 7 and illustrated in figure 4, we estimate insignificant and small coefficients for β_{-4} through β_{-2}, which alleviates these concerns. Interestingly, we estimate a *positive* value for β_{-1}, which may indicate that employers increase recruitment efforts in anticipation of losing credit checks as a screening device. Following the ban, the coefficients become both economically and statistically negative within two quarters and remain so, even beyond a year after the ban is implemented. The coefficient $\beta_{>4}$ captures the change in vacancies for treated counties, relative to untreated, beyond the first year and is close to our baseline estimate that ignored dynamics.

Finally, we leverage occupational variation in exemptions from bans in the dynamic framework. For the exempt group of occupations, there is no significant difference in vacancies before or after the ban, as expected. Affected occupations, however, experience a significant fall in vacancies, which persists after the first year. As with the unconditional estimates, there is no significant difference for affected occupations before the ban. Figure 5 presents this differential effect on exempt and affected industries dynamically. These estimates further suggest that employer-credit check bans cause a decline in job creation in occupations where they are enforced.

c) Effects on Other Labor Market Variables

We prefer vacancies as our labor market outcome because it is most directly related to labor demand and we can leverage occupational variation in ban coverage, but policymakers may be more concerned with other labor market variables, such as the unemployment rate, job-

---

[19] Some examples include Bertrand and Mullainathan (1999 and 2003) in the context of antitakeover legislation and Meer and West (2016) in the study of minimum wage legislation.

finding rate, or job-separation rate. Furthermore, these aggregates are determined jointly in equilibrium, so provide a holistic view of labor-market changes following the ban.

In table 8, we report how the unemployment rate changes post ban, both in the nationwide sample and the adjacent county sample. As seen in columns (1) and (4), there is a small positive, though statistically insignificant, change in the unemployment rate following a ban. The unemployment rate may not respond to the ban for various reasons. For example, the unemployed may search harder after the ban (because, for example, they have less access to debt and therefore cannot smooth their consumption as well). Whatever the reason, the fact that the unemployment rate does not *fall* substantially, while vacancies decline, means that the labor market slackens (the ratio of job openings to job seekers falls).

Moving to labor market flows, we would expect a decline in market tightness to generate a lower job-finding rate. We may also expect some change in the separation rate, though this could move in either direction. For example, if firms are less able to screen for good matches ex-ante, then there may be more workers fired after a short spell of employment. On the other hand, if workers know that they will have a harder time finding work upon quitting an existing job then we may see a decline in separations. We therefore estimate a state-level regression using finding and separation rates as the dependent variables, the results of which are seen in columns (2) and (3) of table 8.[20] The job-finding rate falls by 2.7% post ban (this decline is marginally statistically significant with Z-stat of -1.5), which is consistent with declining market tightness, while the separation rate rises by a similar amount (although this estimate is less precise).

d) Labor Market Effects by County-level Subprime Rates

We find evidence that employer credit check bans have significant adverse effects in the labor market. Even if these bans have a negative effect on average, however, lawmakers (and their constituents) may be in favor of a ban if it helps regions or individuals with bad credit. For the labor market, we estimate whether this equity-efficiency tradeoff applies using the following county-level regression:

$$L_{i,t} = \alpha_i + \gamma_{j,t} + \beta_0 \, Fraction \, Subprime_{i,t} + \beta_1 \, Ban_{i,t} {}^* Fraction \, Subprime_{i,t} + \varepsilon_{i,t}, \qquad (3)$$

---

[20] We use state-level data for these rates because they are not well measured at the county level.

Here again, i indexes the county, and α_i are county-level fixed effects. The dependent variable $L_{i,t}$ stands for either the log of vacancies or the level of unemployment rate. Because we now have within-state variation in the treatment (the interaction of the ban with the fraction of subprime borrowers in the county) we can introduce state-time fixed effects. To do so, we index states by j so that $\gamma_{j,t}$ are state-time effects. In this empirical specification, the credit check ban is interacted with a county's fraction of borrowers with a credit score less than 620 in county i and time t, denoted here by *Fraction Subprime*. We also adapt the distributed-lags specification expressed in equation (2) in this context by replacing the leads and lags of the $Ban_{i,t}$ variables in the interaction term. Table 9 displays the regression results for equation (3) and its distributed-lags version for both dependent variables.

The estimated effect reported for vacancies in column (1) of table 9 is negative for the interaction term. Although the estimates are statistically insignificant at traditional levels, the point estimate implies that bans are actually worse for counties with many subprime households. Moreover, the dynamic specification reported for all occupations in column (2) confirms this negative relationship, this time significantly, for every horizon beyond a quarter after the ban is enacted. The distinction between exempt and affected groups also holds in this specification (columns (3) and (4)), suggesting that bans cause a larger decline in vacancies with many subprime residents. The last two columns of table 9 show that the negative effects on the unemployment rate are exacerbated for counties with more individuals with subprime credit.

To give some context to the magnitude of the decline in vacancies and the rise in unemployment rate vis-à-vis the fraction of subprime borrowers in a county, we plot in figure 6 the interquartile range difference predicted by the regression output in table 9. The range of county subprime shares in table 3 gives context for this estimate: the interquartile difference is 15 percentage points. Therefore, as a state bans employer credit checks, a county in the 75th percentile (which has a 34 percent subprime rate) would start experiencing an 8 percent fall in vacancies relative to a county in the 25th percentile (where 19 percent of residents are subprime), which gradually rises to 12 percent by the third quarter after the ban. This difference remains about 12 percent beyond the first year (figure 6). These post-treatment effects are statistically significant for the most part, even though prior to the ban these counties do not show any discernable difference in terms of labor demand.

As with the previous unemployment rate estimates, the unemployment rate does not respond much more strongly for counties with high subprime rates following the ban (though this response is significant) and is somewhat higher in these counties prior to the ban, as seen in column (6) of table 9. This implies that labor market tightness falls disproportionately more in counties with high shares of subprime residents, since vacancies fall more but the unemployment rate does not. While we cannot estimate the effect of subprime shares on the job-finding rate using our state-level data, these estimates suggest that finding rates fall disproportionately in counties with more subprime residents.

In summary, there is no evidence that employer-credit check bans benefit counties with large shares of subprime workers. If anything, credit check bans seem to adversely affect labor markets in these counties, relative to the average.

e) <u>Credit Market Outcomes</u>

The effects of the credit check bans on credit market outcomes has not been studied prior to this paper. We follow a similar empirical strategy as with above and extend our analysis to the credit panel to estimate borrower-level changes post ban. We run the following panel regression:

$$Y_{i,j,t} = \alpha_i + \gamma_t + \theta_j + \beta_0\, Ban_{j,t} + \beta_1\, Subprime_{i,t} + \beta_2\, Ban_{j,t}{}^*\, Subprime_{i,t} + \varepsilon_{i,t}, \quad (4)$$

In equation (4), we are looking for the effects of the ban on the credit market outcome variable $Y_{i,j,t}$ for an individual i in county j at time t. This specification controls for individual (α_i), county (γ_t), and time (θ_j) fixed-effects. We are interested in credit access and repayment decisions, so we estimate this model using new inquiries for credit over the previous three months (*inquiries*) and the fraction of accounts that are delinquent (*delinquencies*). The interaction term $Subprime_{i,t}$ indicates whether the borrower has a credit score below 620 at that time.

Table 10 reports these estimates using the FRBNY CCP/Equifax. These data are measured quarterly, which allows us to align the credit ban flag accurately since many bans became enforceable at the start of a quarter. The first column of table 10 presents the results for the outcome variable *inquiries*. Although a borrower's subprime status increases the number of inquiries, residing in a ban state significantly decreases the number of inquiries for subprime

borrowers. Total inquiries are a proxy for credit access, so the ban could lead to borrowers' becoming more credit constrained. The dynamic response of inquiries shows smaller but significant declines up until a year post ban, as seen in column (2). Unfortunately, we cannot tell if inquiries fell because borrowers requested fewer lines of credit or because financial institutions made fewer offers to residents of treated counties.

The third column of table 10 reports the change in repayment behavior following a ban. Unconditionally, there is a small decline in delinquencies post ban (-0.6%), but the delinquency rate for subprime borrowers rises substantially. The subprime delinquency rate is 5.8% higher in areas with the credit check ban, a number that is large in economic terms, since 46% of accounts are delinquent for these borrowers. This decline arises only after the ban becomes enforceable, as seen in column (4), and remains significantly negative even after a year.

As with inquiries, our data are silent about why subprime borrowers become delinquent on more accounts. This increase is consistent with either borrowers *choosing* to cease payments or being less able to repay their debts. While we do not observe income or job status for these borrowers, Friedberg, Hynes, and Pattison (2017)'s finding that financially distressed workers have shorter unemployment durations post ban suggests that the rise in delinquencies for subprime borrowers may be strategic.

Previous studies (Ballance, Clifford and Shoag, 2016; Bartik and Nelson, 2016) provided evidence that workers from minority groups were especially hurt by employer credit check bans. A natural question, then, is whether these groups were especially affected in the credit market. Previous research could not directly address this question because credit reports exclude demographic information. We overcome this limitation with a new dataset, the TransUnion-Epsilon merged panel, which has both credit report data (from TransUnion rather than Equifax) and demographics from Epsilon, a marketing firm. Tables 11 and 12 show the results for total inquiries and rate of total loans delinquent, respectively, this time broken down by race. Column 1 of each table shows the results for the full sample. As with the FRBNY CCP/Equifax data, the coefficient on the interaction variable is negative for the total inquiries, implying a net reduction in new credit.[21] The estimated effect of the ban for subprime workers is similar across Whites,

---

[21] The magnitude of the effect is larger in the TransUnion data because it is an annual panel rather than the quarterly Equifax.

Blacks, and Hispanics, at -0.486 inquiries, while the unconditional effect of the ban is slightly positive, at 0.085 inquiries (these point estimates are similar across groups).

The effect of employer credit bans for delinquency rates exhibit interesting differences across racial groups. The net effect of the ban is a 3.12% increase in delinquency rates for subprime individuals, an economically significant increase— on average, subprime individuals are delinquent on 46% of loans (see table 4). Somewhat surprisingly, the increase is driven entirely by Whites and Hispanics: For Blacks, the point estimate is both economically and statistically insignificant. Since previous research has estimated worse labor market effects for black workers, the fact that their delinquency rate is unaffected by the ban may indicate that they are made no better off by an employer credit check ban, while Whites and Hispanics are made worse off. As in the Equifax panel, the unconditional effect on delinquencies is an order of magnitude smaller for Blacks than other groups, and is statistically insignificant.[22]

We are the first to study the spillover of employer credit check bans into the credit market, and our estimates highlight the importance of a holistic view of the consequences from such bans. On net, our estimates indicate that employer credit check bans have negative effects on credit markets, specifically for subprime borrowers.

4) Conclusion

In 2007, the state of Washington was the first to pass a ban on credit checks for employers. Ten more states have adopted such policies since, and federal legislation has been proposed. These bans are intended to break a cycle in which limited employment opportunities cause financial distress, which further reduces labor market opportunities. The deep downturn in economic activity and severe housing market crisis experienced during the Great Recession provided policymakers with a strong motivation to introduce such legislation. Using both labor and credit market data, we show that these laws have likely reduced job creation, while being associated with reduced credit access and higher delinquency rates.

Our estimates are consistent with the predictions of a micro-founded model (Corbae & Glover (2017)). Vacancy postings fall after the introduction of the credit check ban for affected occupations, which is consistent with a reduction in labor demand due employers losing a cheap

---

[22] The results for Blacks remain positive and become significant at the 10% level if we trim the bottom and top 1%.

screening device (credit reports). We also find evidence that employer credit check bans have negative effects on subprime borrowers, who experience a decline in credit inquiries and a rise in delinquencies. While our data cannot definitively test the mechanism from Corbae & Glover, the fact that the changes we estimate are accompanied by widespread welfare losses in their model suggests that policymakers should be cautious when banning employer credit checks.

References

Ballance, Joshua, Robert Clifford and Daniel Shoag. 2016. "No More Credit Score; Employer Credit Check Bans and Signal Substitution." Federal Reserve Bank of Boston Working Paper.

Bartik, Alexander W., and Scott T. Nelson. 2016. "Credit Reports as Résumés: The Incidence of Pre-Employment Credit Screening," mimeo.

Bertrand, Marianne and Sendhil Mullainathan. 1999. "Is there Discretion in Wage Setting? A Test Using Takeover Legislation" *Rand Journal of Economics*, 30-3:535:54.

Bertrand, Marianne and Sendhil Mullainathan. 2003. "Enjoying the Quiet Life? Corporate Governance and Managerial Preferences," *Journal of Political Economy*, 111-3:1043-75.

Bos, Marieke, Emily Breza and Andres Liberman. 2015. "The Labor Market Effects of Credit Market Information," *Review of Financial Studies,* Forthcoming.

Cajner, Tomaz and David D. Ratner (2016). "A Cautionary Note on the Help Wanted Online Data," FEDS Notes. Washington: Board of Governors of the Federal Reserve System, June 23, 2016, http://dx.doi.org/10.17016/2380-7172.1795.

Conference Board, The. (2017). *The Conference Board Help Wanted Online® (HWOL) [Data file and documentation]*. https://www.conference-board.org/data/helpwantedonline.cfm

Corbae, Dean and Andew Glover. 2017. "Employer Credit Checks: Poverty Traps Versus Matching Efficiency," mimeo.

Demos. 2012. "Discredited: How Employer Credit Checks Keep Qualified Workers Out of a Job."

Dobbie, Will, Paul Goldsmith-Pinkham, Neale Mahoney and Jae Song. 2016. "Bad Credit, No Problem? Credit and Labor Market Consequences of Bad Credit Reports." NBER Working Paper 22711.

Donaldson, Jason R., Giorgia Piacentino and Anjan Thakor. 2017. "Household Debt and Unemployment," *Journal of Finance*, Forthcoming.

Dube, Arindrajit, T. William Lester, and Michael Reich. "Minimum Wage Effects across State Borders: Estimates Using Contiguous Counties." *The Review of Economics and Statistics,* 92.4 (2010): 945-964.

Friedberg, Leora, Richard M. Hynes, and Nathaniel Pattison. 2017. "Who Benefits from Credit Report Bans?" mimeo.

Herkenhoff, Kyle. 2015. "The Impact of Consumer Credit Access on Unemployment," mimeo.

Herkenhoff, Kyle, Gordon Phillips and Ethan Cohen-Cole. 2016. "How Credit Constraints Impact Job-Finding Rates, Sorting & Aggregate Output," mimeo.

Meer, Jonathan and Jeremey West. 2016. "Effects of the Minimum Wage on Employment Dynamics," *Journal of Human Resources*, 51-2:500-22.

Nekarda, Christopher J. 2009. "A Longitudinal Analysis of the Current Population Survey: Assessing the Cyclical Bias of Geographic Mobility," mimeo.

Sahin, Aysegul, J. Song, G. Topa, G. Violante. 2014. "Mismatch Unemployment" *American Economic Review,* 104.11: 3529-64.

Society of Human Resource Managers. 2012. "Background Checking: The Use of Credit Background Checks in Hiring Decisions."

Vasel, Kathryn. 2016. "No More Credit Checks for NYC Job Seekers." CNN Money. CNN, 17 April 2015. Web. Accessed 24 March http://money.cnn.com/2015/04/17/pf/jobs/new-york-city-credit-check-employment/.

Figure 1: Credit Check Ban Legislation
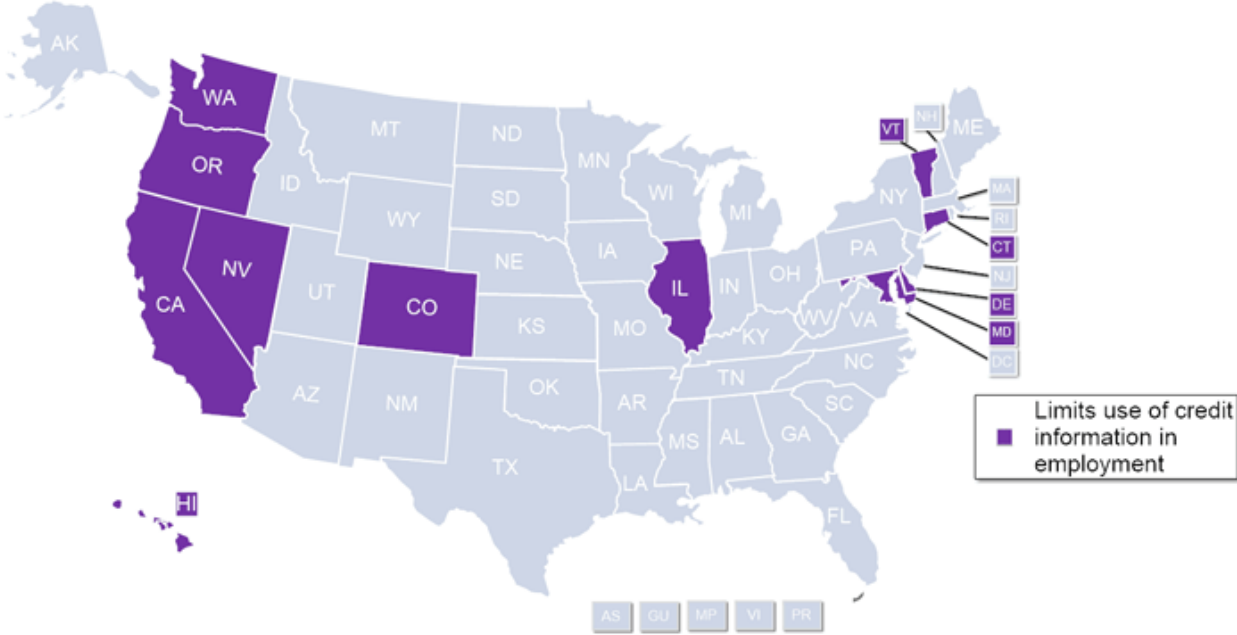
Source: National Conference of State Legislatures

Figure 2: Labor Market Outcomes across States and Credit Check Bans
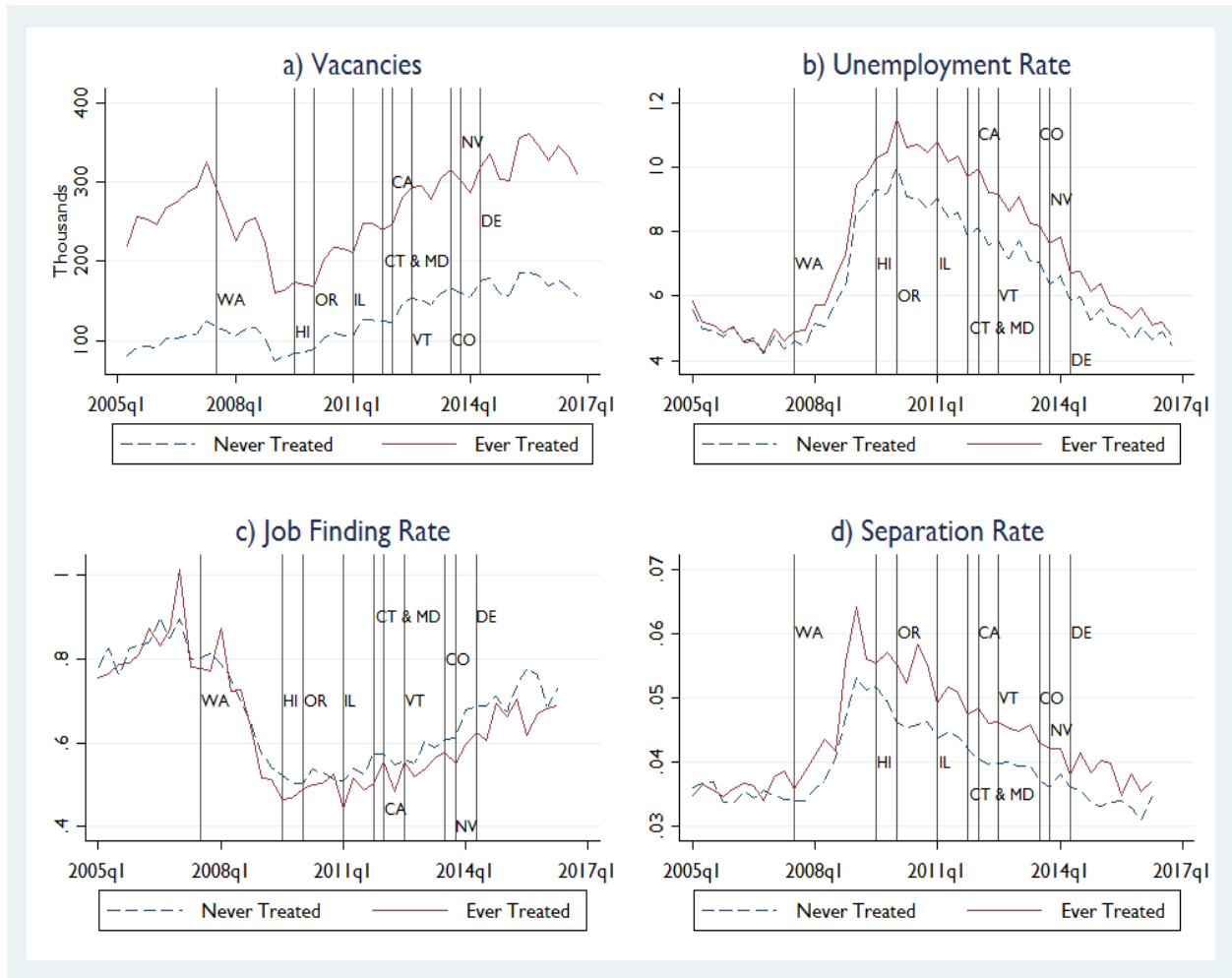
Source: The data come from HWOL and CPS state-level aggregates, in which each state is weighted by its labor force. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect.

Figure 3: Poverty, Income and Credit Market Outcomes across States and the Credit Check Bans
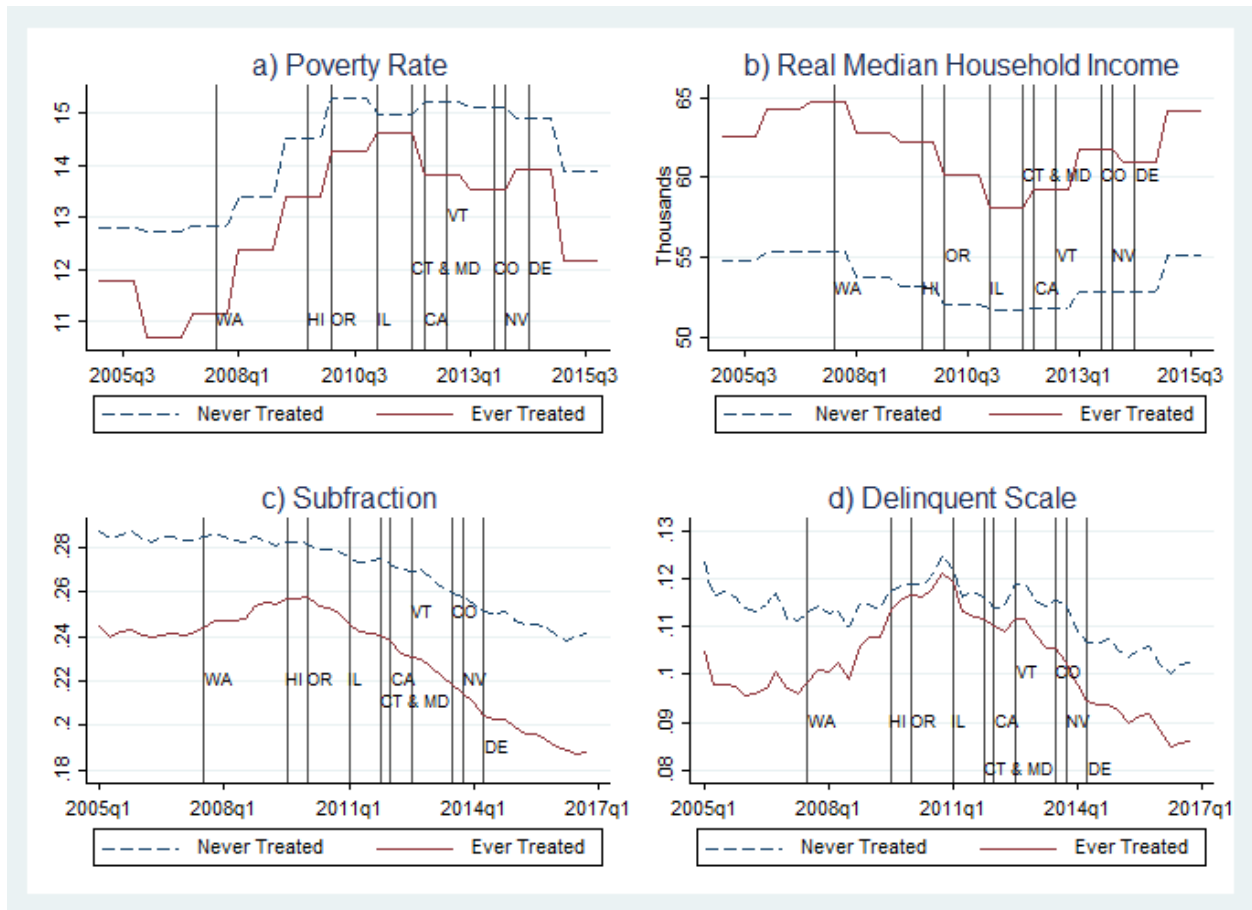
Source: The data for Poverty Rate and Median Household Income come from Small Area Income and Poverty Estimates (SAIPE) conducted by the Census Bureau. The fraction of subprime borrowers (Subfraction) and the percentage of loans that are delinquent come from FRBNY CCP/Equifax. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect.

Figure 4: Dynamics of Vacancies Around Ban
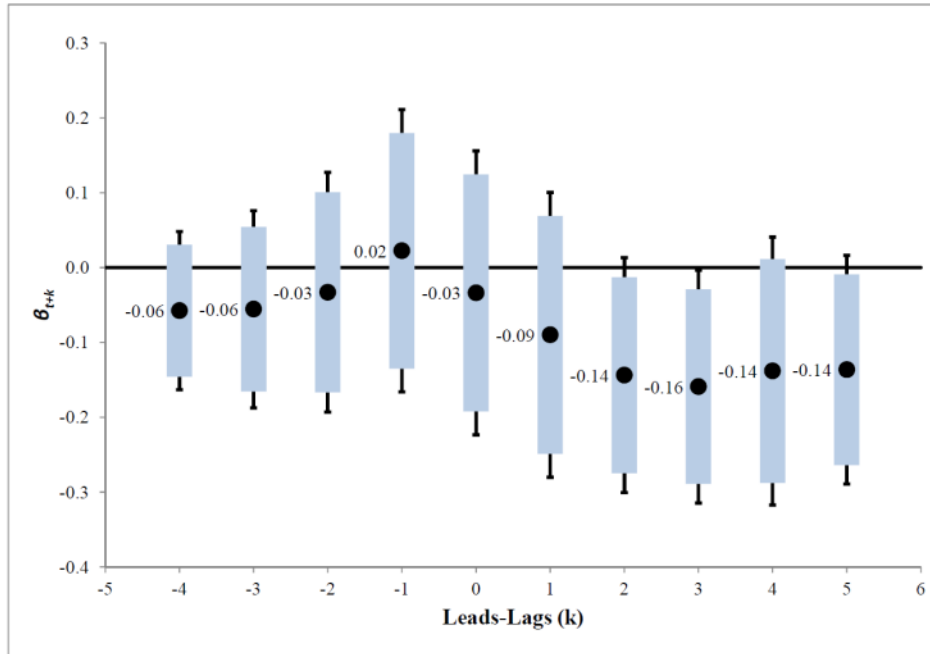
Note: Shaded areas and bars represent 90 percent and 95 percent confidence bands around the coefficient estimates.

Figure 5: Dynamics of Vacancies Around Ban by Occupation Type

a) Exempt Occupations
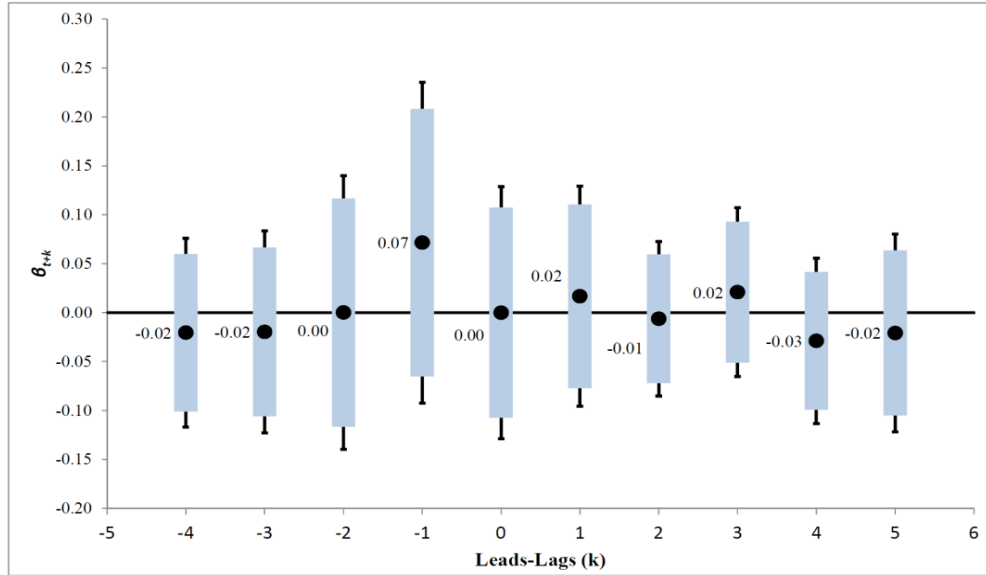
b) Affected Occupations
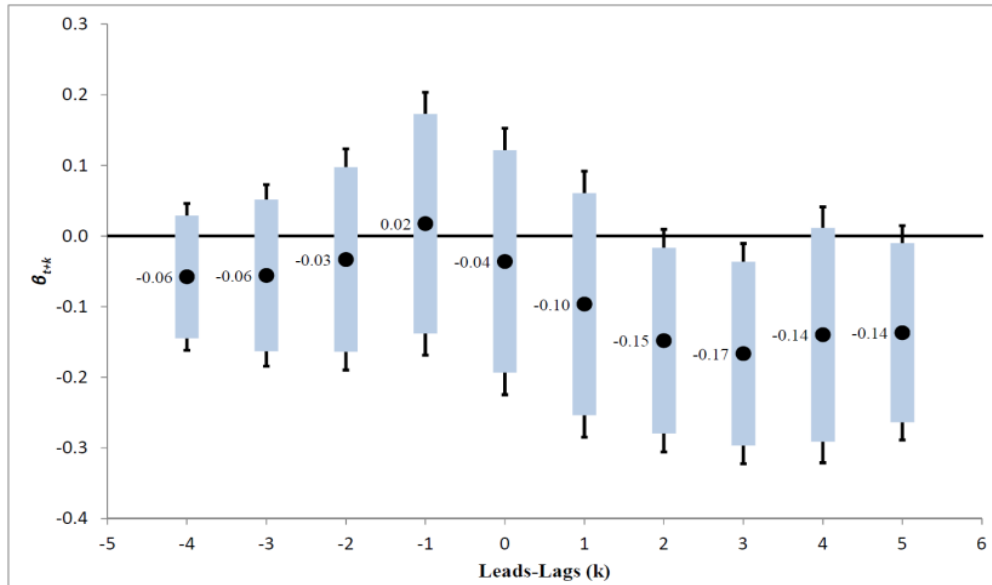
Note: Shaded areas and bars represent 90 and 95 percent confidence bands around the coefficient estimates, respectively. Exempt occupations include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33).

Figure 6: Dynamics of Vacancies Around Ban Interacted with Subprime Rate
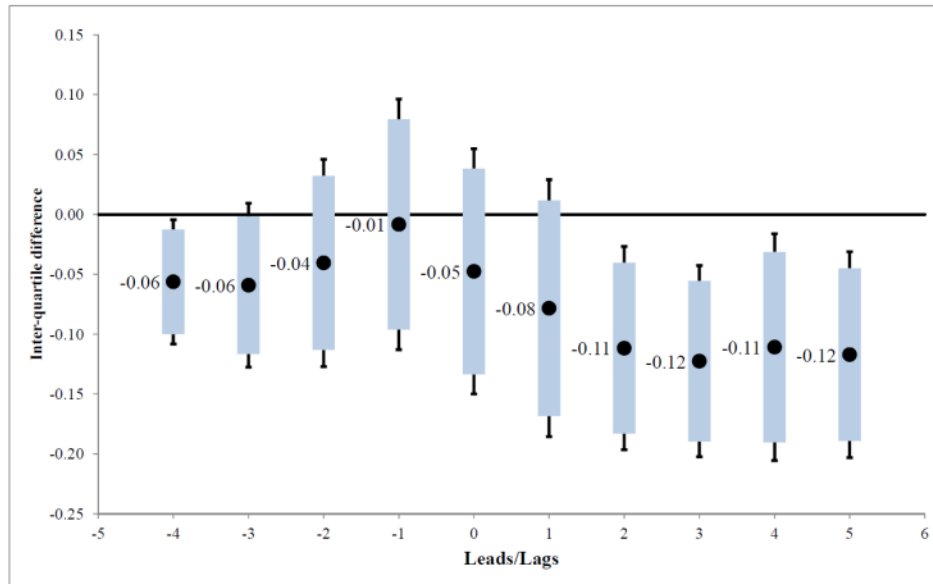
Note: Point estimates indicate the predicted difference in the dependent variable between an average county in the 75th percentile and one in the 25th percentile (in terms of the variable *fraction subprime*). Shaded areas and bars represent 90 percent and 95 percent confidence bands around the coefficient estimates.

Table 1: Dates When Bans Went into Effect

State	Date of Effective Law Change	Neighboring States
CA	1/1/12	NV, AZ, OR
CO	1/7/13	UT, WY, NE, KS, OK, NM
CT	1/10/11	MA, NY, RI
DE	8/5/14	MD, NJ, PA
HI	1/7/09	none
IL	1/1/11	IN, KY, MO, IA, WI
MD	1/10/11	DE, PA, VA, WV
NV	1/10/13	AZ, CA, ID, OR, UT
OR	29/3/10	CA,ID, NV,WA
VT	1/7/12	MA,NH, NY
WA	22/7/07	ID, OR

Table 2: Distribution of Labor Market Variables and Ban Flag – County Level

Year	Obs.	Counties with Law in Effect	States with Law in Effect	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
		Law Flag		Log(Vacancies+1)			Log(Vacancies)			Unemployment Rate		
2005	3,141	0	0	9,423	4.22	2.09	9,284	4.22	2.14	12,546	5.46	2.00
2006	3,141	0	0	12,564	4.31	2.11	12,438	4.28	2.16	12,546	4.97	1.88
2007	3,141	39	1	12,564	4.41	2.15	12,441	4.38	2.20	12,560	4.89	1.87
2008	3,141	39	1	12,564	4.49	2.11	12,459	4.47	2.16	12,560	5.83	2.25
2009	3,141	44	2	12,564	4.46	1.99	12,463	4.44	2.04	12,560	9.07	3.35
2010	3,140	80	3	12,560	4.62	2.01	12,490	4.60	2.05	12,556	9.38	3.31
2011	3,138	214	6	12,552	4.79	2.02	12,484	4.78	2.06	12,548	8.73	3.13
2012	3,138	286	8	12,552	5.05	1.97	12,501	5.04	1.99	12,548	7.86	2.89
2013	3,138	367	10	12,552	5.17	1.95	12,519	5.16	1.98	12,548	7.38	2.80
2014	3,137	370	11	12,551	5.25	1.96	12,529	5.24	1.99	12,547	6.25	2.47
2015	3,137	370	11	12,548	5.38	1.92	12,529	5.37	1.94	12,544	5.53	2.14
2016	3,137	370	11	12,548	5.34	1.90	12,532	5.33	1.93	12,544	5.25	1.99
All Years				147,542	4.80	2.05	146,669	4.79	2.09	150,607	6.72	3.03

Table 3: Distribution of Subprime Borrowers – County Level

				Fraction Subprime			
Year	Obs.	Mean	25th pct	Median	75th pct	95th pct	Std. dev.
2005	12,560	0.29	0.20	0.27	0.36	0.5	0.12
2006	12,560	0.29	0.20	0.27	0.36	0.5	0.12
2007	12,559	0.28	0.20	0.27	0.36	0.49	0.12
2008	12,556	0.28	0.20	0.27	0.35	0.48	0.11
2009	12,556	0.28	0.20	0.27	0.35	0.47	0.11
2010	12,548	0.26	0.20	0.26	0.34	0.46	0.11
2011	12,548	0.27	0.19	0.25	0.33	0.46	0.11
2012	12,548	0.26	0.19	0.25	0.33	0.46	0.11
2013	12,546	0.26	0.18	0.24	0.32	0.45	0.11
2014	12,544	0.25	0.17	0.24	0.32	0.44	0.11
2015	12,544	0.24	0.17	0.23	0.31	0.44	0.11
2016	12,544	0.24	0.17	0.23	0.31	0.43	0.11
All years	150,613	0.27	0.19	0.26	0.34	0.47	0.11

Table 4: Credit Panel Summary Statistics, Equifax and TransUnion

Panel A: All Borrowers

Total Inquires

	Obs.	Mean	Median	Std. dev
Equifax (All)	30,011,039	0.590	0	0.983
TransUnion (All)	8,360,807	0.759	0	1.364
TransUnion (White)	4,902,086	0.688	0	1.299
TransUnion (Black)	297,091	1.003	0	1.576
TransUnion (Hispanic)	666,081	0.924	0	1.504

Total Loans Delinquent

	Obs.	Mean	Median	Std. dev
Equifax (All)	37,203,721	0.126	0	0.292
TransUnion (All)	6,996,168	0.098	0	0.282
TransUnion (White)	4,289,566	0.075	0	0.247
TransUnion (Black)	229,326	0.186	0	0.367
TransUnion (Hispanic)	555,662	0.134	0	0.32

Panel B: Subprime Borrowers

Total Inquires

	Obs.	Mean	Median	Std. dev
Equifax (All)	9,442,835	0.823	0	1.200
TransUnion (All)	1,958,371	1.303	1	1.757
TransUnion (White)	853,181	1.330	1	1.790
TransUnion (Black)	131,385	1.396	1	1.797
TransUnion (Hispanic)	184,111	1.385	1	1.802

Total Loans Delinquent

	Obs.	Mean	Median	Std. dev
Equifax (All)	9,119,384	0.455	0.400	0.405
TransUnion (All)	1,230,827	0.457	0.333	0.477
TransUnion (White)	580,403	0.441	0.333	0.471
TransUnion (Black)	83,024	0.447	0.333	0.470
TransUnion (Hispanic)	125,204	0.471	0.333	0.477

Panel C: Credit Scores

Equifax Risk Score and TransUnion VantageScore*

	Obs	Mean	Median	Std. dev
Equifax (All)	41,942,119	693	713	107
TransUnion (All)	8,344,097	733	733	125
TransUnion (White)	4,902,086	756	763	122
TransUnion (Black)	297,091	664	638	116
TransUnion (Hispanic)	666,081	701	690	112

*During the sample years 2005-2014, the Equifax Risk Score ranged from 280-850 and the TransUnion Vantage Score ranged from 501-990.

Table 5: Effects of Credit Check Bans on Labor Demand - Full Sample Baseline Specification

	Dependent Variable: Log (Vacancies +1)		
	(1)	(2)	(3)
	All occupations Total	Exempt occupations Total	Affected occupations Total
Credit check ban	-0.124*	-0.017	-0.126*
	(0.065)	(0.040)	(0.065)
County fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Number of clusters (states)	51	51	51
Observations	147,542	147,542	147,542
R-squared	0.965	0.946	0.964

Standard errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for various dependent variables for each county i at time t (quarterly.) The dependent variable in column (1) is the log of the Conference Board's HWOL data on total vacancies, our proxy measure for labor demand. Column (2) reports Exempt occupations which include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33). Column (3) reports Affected occupations, which includes all other occupations. Credit check ban is 1 for counties in states that pass the credit check ban and zero otherwise. The county fixed effects and time (quarterly) fixed effects help remove unobserved heterogeniety such as variation in labor supply. Standard errors are clustered by state.

Table 6: Effects of Credit Check Bans on Labor Demand - Adjacent Border County Specification

	Dependent Variable: Log (Vacancies +1)		
	(1)	(2)	(3)
	All occupations	Exempt occupations	Affected occupations
Credit check ban	-0.095**	-0.031	-0.097**
	(0.047)	(0.043)	(0.047)
County fixed effects	Yes	Yes	Yes
County pair*time fixed effects	Yes	Yes	Yes
Number of clusters (states & borders)	256	256	256
Observations	70,218	70,218	70,218
R-squared	0.983	0.975	0.983

Two-way standard errors clustered at the state level and border level.

*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for various dependent variables for each county *i* at time *t* (quarterly.) The dependent variable in column (1) is the log of the Conference Board's HWOL data on total vacancies, our proxy measure for labor demand. Column (2) reports Exempt occupations which include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33). Column (3) reports Affected occupations, which includes all other occupations. Credit check ban is 1 for counties in States that pass the credit check ban and zero otherwise. The county fixed effects and the county-pair by time (quarterly) fixed effects help remove unobserved heterogeniety such as variation in labor supply. Two-way standard errors are clustered by state and border.

Table 7: Dynamic Effects of Credit Check Bans on Labor Demand

	Dependent Variable: Log (Vacancies +1)		
	(1)	(2)	(3)
	All occupations	Exempt occupations	Affected occupations
Credit check ban, $t-4$	-0.058	-0.021	-0.058
	(0.053)	(0.048)	(0.052)
Credit check ban, $t-3$	-0.056	-0.020	-0.056
	(0.066)	(0.051)	(0.064)
Credit check ban, $t-2$	-0.033	0.000	-0.033
	(0.080)	(0.070)	(0.078)
Credit check ban, $t-1$	0.022	0.072	0.017
	(0.094)	(0.082)	(0.093)
Credit check ban, t	-0.034	-0.000	-0.036
	(0.094)	(0.064)	(0.094)
Credit check ban, $1+1$	-0.090	0.017	-0.097
	(0.095)	(0.056)	(0.094)
Credit check ban, $t+2$	-0.144*	-0.006	-0.148*
	(0.078)	(0.039)	(0.078)
Credit check ban, $t+3$	-0.159**	0.021	-0.167**
	(0.077)	(0.043)	(0.078)
Credit check ban, $t+4$	-0.138	-0.029	-0.140
	(0.089)	(0.042)	(0.090)
Credit check ban, $t>4$	-0.136*	-0.021	-0.137*
	(0.076)	(0.050)	(0.076)
County fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Number of clusters (states)	51	51	51
Observations	147,542	147,542	147,542
R-squared	0.965	0.947	0.965

Standard Errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1
This table reports OLS regressions for various dependent variables for each county i at time t (quarterly.) The dependent variable in column (1) is the log of the Conference Board's HWOL data on total vacancies, our proxy measure for labor demand. Column (2) focuses on new vacancies instead of total vacancies reported. Column (3) reports Exempt occupations which include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33). Column (4) reports Affected occupations, which includes all other occupations. Credit check ban is 1 for counties in states that pass the credit check ban and remains 1 for the remaining time periods, and zero otherwise. The county fixed effects and time (quarterly) fixed effects help remove unobserved heterogeniety such as variation in labor supply. Standard errors are clustered by state.

Table 8: Changes in Other Labor Market Outcomes Post Ban

	Nationwide Samples			Adjacent County
	Unemployment Rate	Seperation Rate	Finding Rate	Unemployment Rate
	(1)	(2)	(3)	(4)
Credit check ban	0.150	0.027	-0.027	0.220
	(0.186)	(0.021)	(0.018)	(0.223)
County / State Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	
County pair*time fixed effects				Yes
Number of clusters (states or states & borders)	51	51	51	256
Observations	150,607	150,607	71,704	71,704
R-squared	0.826	0.826	0.932	0.931

*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for various labor market measurements for each county i at time t (quarterly). Column (1) reports the regression results based on specification (1a) in the text for dependent variable, unemployment rate. Columns (2) and (3) report the regression coefficients from a specification of (1a) with state-level (log) separation and finding rates as the dependent variable. Column (4) reports regression results for unemployment rate focusing on only adjacent-border counties, using specification (1b). Credit check ban is 1 for counties in States that pass the credit check ban and zero otherwise. The county fixed effects, time (quarterly) fixed effects and the county-pair by time (quarterly) fixed effects help remove unobserved heterogeniety such as variation in labor supply. Standard errors are clusterd by state except column (4) in which two-way standard errors are clustered by state and border.

Table 9: Effects of the Credit Check Bans and Subprime Borrowers

Dependent Variable	Log (Vacancies + 1)				Unemployment Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
	All Occupations		Exempt Occupations	Affected Occupations		
Fraction Subprime * Ban	-0.003				0.022***	
	(0.004)				(0.008)	
Fraction subprime	-0.000	-0.001	0.000	-0.001	0.005**	0.009**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Subprime * Ban t-4		-0.003	-0.001	-0.003		0.048***
		(0.002)	(0.002)	(0.002)		(0.016)
Subprime * Ban t-3		-0.003	-0.001	-0.003		0.032**
		(0.002)	(0.002)	(0.002)		(0.015)
Subprime * Ban t-2		-0.002	-0.000	-0.002		0.032**
		(0.003)	(0.003)	(0.003)		(0.012)
Subprime * Ban t-1		0.001	0.003	0.000		0.030*
		(0.004)	(0.003)	(0.004)		(0.017)
Subprime * Ban t		-0.002	-0.001	-0.002		0.025
		(0.004)	(0.003)	(0.004)		(0.021)
Subprime * Ban t+1		-0.004	0.001	-0.004		0.023
		(0.004)	(0.002)	(0.004)		(0.016)
Subprime * Ban t+2		-0.006**	-0.000	-0.007**		0.032***
		(0.003)	(0.001)	(0.003)		(0.008)
Subprime * Ban t+3		-0.007**	0.001	-0.007**		0.028***
		(0.003)	(0.002)	(0.003)		(0.010)
Subprime * Ban t+4		-0.006*	-0.002	-0.006*		0.012
		(0.003)	(0.002)	(0.003)		(0.013)
Subprime * Ban $> t$+4		-0.007**	-0.001	-0.007**		0.015
		(0.003)	(0.002)	(0.003)		(0.009)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State*time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	51	51	51	51	51	51
Observations	147,426	147,473	147,473	147,473	150,537	150,585
R-squared	0.971	0.965	0.947	0.965	0.906	0.826

Standard Errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for vancancy and unemployment rates for each county i at time t (quarterly.) Columns (1) and (5) display regression results following the specification in equation (3), for vacancies and unemployment rate, respectively. The remaining columns incorporate the variation in the subprime fraction of the borrowers in a county to implement the distributed lag specification expressed in equation (2). Fraction subprime (or subprime) is the fraction of borrowers with a current credit score below 620 in county i at time t. Ban stands for credit check ban and is 1 for counties in States that pass the credit check ban and zero otherwise. The county fixed effects, and the state by time (quarterly) fixed effects help remove unobserved heterogeniety such as variation in labor supply. Standard errors are clusterd by state.

Table 10: Dynamics of Equifax Credit Panel Inquiries and Delinquencies Around Ban

Dependent Variable	Total inquiries		Rate of total loans delinquent	
	(1)	(2)	(3)	(4)
Credit check ban*Subprime	-0.116***		0.0581***	
	(0.011)		(0.007)	
Credit check ban	0.028**		-0.00637**	
	(0.013)		(0.003)	
Subprime	0.066***		0.260***	
	(0.008)		(0.002)	
Subprime * Ban $t-4$		-0.009		0.003
		(0.007)		(0.006)
Subprime * Ban $t-3$		-0.007		-0.001
		(0.007)		(0.004)
Subprime * Ban $t-2$		-0.027***		-0.003**
		(0.008)		(0.001)
Subprime * Ban t		0.007		0.004**
		(0.014)		(0.002)
Subprime * Ban $t+1$		-0.018		0.002
		(0.021)		(0.003)
Subprime * Ban $t+2$		-0.049*		0.007**
		(0.027)		(0.004)
Subprime * Ban $t+3$		-0.042*		0.010**
		(0.025)		(0.004)
Subprime * Ban $t+4$		-0.043*		0.010***
		(0.024)		(0.003)
Subprime * Ban $> t+4$		-0.035		0.016*
		(0.048)		(0.008)
County fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Borrower fixed effects	Yes	Yes	Yes	Yes
Number of clusters	51	51	51	51
Observations	29,974,569	28,804,982	37,147,443	37,146,820
R-squared	0.158	0.194	0.660	0.671

Standard Errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for individual i in county j at time t of the effect of the credit check ban on inquiries and delinquencies using the Quarterly New York Fed Equifax Consumer Credit Panel. Columns (1) and (3) estimate the regression in equation (4). Columns (2) and (4) report the coefficients on the interaction of a borrower's subprime status and the distributed lags of the ban indicator in which we have normalized the t-1 coefficients to zero. Subprime is equal to 1 if the individual i has a current credit score below 620 at time t. Ban stands for credit check ban and is 1 for counties in States that pass the credit check ban and zero otherwise. The county fixed effects, time (quarterly) fixed effects and individual borrower fixed effects help remove unobserved heterogeniety such as variation in credit usage. Standard errors are clusterd by state.

Table 11: TransUnion/Epsilon Credit Panel Total Inquiries by Race

Dependent Variable	Total Inquiries			
	All	White	Black	Hispanic
Credit check ban*Subprime	-0.486***	-0.479***	-0.481***	-0.459***
	(0.042)	(0.047)	(0.050)	(0.022)
Credit check ban	0.085***	0.065***	0.065	0.132***
	(0.020)	(0.019)	(0.040)	(0.035)
Subprime	0.263***	0.259***	0.232***	0.280***
	(0.017)	(0.014)	(0.021)	(0.020)
Borrower fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Number of clusters	51	51	51	51
Observations	8,344,097	4,895,036	295,125	663,454
R-squared	0.337	0.338	0.383	0.344

Standard Errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for individual i in county j at time t of the effect of the credit check ban on inquiries using the TransUnion/Epsilon Credit Panel. Columns (1) reports results for the full sample. Columns (2), (3) and (4) report the coefficients for White, Black and Hispanic borrowers (respectively.) Subprime is equal to 1 if the individual i has a current credit score below 620 at time t. Ban stands for credit check ban and is 1 for counties in States that pass the credit check ban and zero otherwise. The Zip code fixed effects, time (year) fixed effects and individual borrower fixed effects help remove unobserved heterogeniety such as variation in credit usage. Standard errors are clusterd by state.

Table 12: TransUnion/Epsilon Credit Panel Total Loans Delinquent by Race

Dependent Variable	Rate of Total Loans Delinquent			
	All	White	Black	Hispanic
Credit check ban*Subprime	0.031***	0.032***	0.003	0.043***
	(0.004)	(0.004)	(0.010)	(0.009)
Credit check ban	-0.002*	-0.003**	0.004	-0.008***
	(0.001)	(0.001)	(0.004)	(0.003)
Subprime	0.350***	0.348***	0.329***	0.352***
	(0.005)	(0.005)	(0.004)	(0.012)
Borrower fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Number of clusters	51	51	51	51
Observations	6,955,336	4,272,570	225,946	551,051
R-squared	0.549	0.537	0.533	0.544

Standard Errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

This table reports OLS regressions for individual i in county j at time t of the effect of the credit check ban on rate of total loans deliquent using the TransUnion/Epsilon Credit Panel. Columns (1) reports results for the full sample. Columns (2), (3) and (4) report the coefficients for White, Black and Hispanic borrowers (respectively.) Subprime is equal to 1 if the individual i has a current credit score below 620 at time t. Ban stands for credit check ban and is 1 for counties in States that pass the credit check ban and zero otherwise. The Zip code fixed effects, time (year) fixed effects and individual borrower fixed effects help remove unobserved heterogeniety such as variation in credit usage. Standard errors are clusterd by state.